ciently detailed to make the construction of the table readily understood, assuming a knowledge of the use of logarithms. The book will prove a handy substitute for more bulky volumes in cases where extreme accuracy is not required, such as computations in chemistry and physics.

LETTERS TO THE EDITOR.

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Panmixia.

MY letter of March 6 commenced with the remark that, without entering into controversy, I proposed to draw attention to the opinions expressed concerning the inheritance of acquired characters by Mr. Darwin. The reasons for my own beliefs on the questions at issue I have given in "The Principles of Biology," § 166, and, with other illustrations, in "The Factors of Organic Evolution." Here it must suffice to say that I have seen no reason to abandon the conclusions there set forth.

Respecting the doctrine of "panmixia," either as enunciated by Prof. Weismann, or as recently presented in modified forms, I will say no more than that I should like to see its adequacy discussed in connection with a specific instance—say the drooping ears of many domesticated animals. "Cats in China, horses in parts of Russia, sheep in Italy and elsewhere, the guinea-pig in Germany, goats and cattle in India, rabbits, pigs, and dogs in all long-civilized countries, have dependent ears."

Here the influence of natural selection is almost wholly excluded; nor can artificial selection be supposed to have operated in most of the cases: save, perhaps, in some pet animals, selection has been carried on to develop other traits. In the cases of most of these creatures, too, artificially fed and often over-fed, it does not seem that individual fates can have been affected by economy of nutrition, either general or special; since there has been no struggle for existence to cause the survival of those in which nutriment was most advantageously distributed. Further, the parts in question are not of such sizes that economy in nutrition of them could sensibly affect the fates of individuals, even had the struggle for existence been going on. Again, it seems that in respect of the ears themselves (though not in respect of their motor muscles) there has been extravagance of nutrition rather than economy of nutrition; since even where selection has been carried on for increasing other traits, the ears have not dwindled but rather increased. Lastly, at the same time that there has been this superfluity of nutrition in the ears themselves, their motor muscles appear to have dwindled either relatively or absolutely—at least relatively, we must suppose, where the weight of the ears has increased, and absolutely where the weight of the ears has not increased.

The question presented by these facts is one in the solution of which the theory of "panmixia" may, I think, be satisfactorily tested; and without expressing any opinion upon the matter myself, I should be glad to see it discussed.

HERBERT SPENCER.

I AM not aware how far Prof. Ray Lankester is disposed to acknowledge his obligations to Prof. Weismann for what I am glad to see he now calls his "anti-Lamarckian" (as distinguished from "pure Darwinian") proclivities. Therefore I do not know how far he professes to be one of "the followers of Prof. Weismann," to whom my previous letter on this subject was addressed. But it seems desirable that I should take some notice of the altogether distinct question which he has now raised—viz. whether, or how far, Prof. Weismann's anti-Lamarckian views were anticipated by Mr. Darwin.

His argument is that Darwin must have been a Lankesterian anti-Lamarckian in disguise; and, more particularly, that "the doctrine of panmixia is recognized and formulated in the last (sixth) edition of the 'Origin of Species' published in 1872."

Taking the most general statement first, Prof. Lankester represents it as not improbable that "when Darwin refers, here and there throughout his works, to a reduced or rudimentary condition of an organ as 'due to disuse,' or 'explained by the effects of disuse,' he does not *necessarily* mean such effects as the Lamarckian second law asserted and assumed (though often he does appear to mean such); but he may mean, and probably had in his mind, the effects of disuse as worked out through panmixia and economy of growth."

Now, here we have a specimen of Prof. Lankester's dialectic at its worst. Truly, with such an interpreter, Darwin "*may*" be made to "mean" anything. First it is represented as seeming "not at all improbable that when Darwin refers" to one principle, "he does not *necessarily* mean" what he says; and then it is concluded that "he may mean, and *probably* had in his mind a totally different principle." Moreover, what is represented as mere references, "here and there throughout his works," are, as all the world knows, one whole and "highly important" (though still subordinate) side of Darwin's system. Yet again, in all passages where the meaning assigned to his term "disuse" is explained, there can be no shadow of ambiguity attaching to it, and everywhere it is alluded to as a principle wholly distinct from the "economy of growth"; while panmixia, as I shall presently prove, is nowhere mentioned at all. This, indeed, is clearly shown even in the passages quoted by Prof. Lankester, and now re-quoted below. For it is there said that, could a certain explanation be found, "then we should be able to understand how an organ which has *become useless* would be rendered, *independently of the effects of disuse*, rudimentary." Obviously, in this context, "the effects of disuse" cannot possibly mean "the effects of disuse as worked out through panmixia and economy of growth": they *can* only mean the direct effects of disuse itself in causing inherited atrophy. And now, lastly, "the effects of disuse" are habitually pointed to by Mr. Darwin in association with the "effects of *increased use*"; and how he can "seem" to have "explained" *these* either by the economy of growth (which he fully recognized), or by panmixia (which he never recognized), I must leave Prof. Lankester to indicate.

It will be observed, from the point last mentioned, that this attempt to read the doctrines of Weismann into the writings of Darwin must equally collapse, whether or not any other human being can be found to follow Prof. Lankester in his commentary on Darwin's "here and there" references to "the effects of disuse": the equally constant and as frequently detailed references to "the effects of the increased use of parts, which I have always maintained to be highly important," are of themselves sufficient to dispose of the Lankesterian gloss. Nevertheless, it remains worth while to see whether there is any shred of evidence in support of the narrower or more particular statement, that the principle of panmixia is to be found "already indicated" in the "Origin of Species." The following are the passages upon which this statement is founded—passages, I may remark, which have certainly neither been "missed" nor "neglected" by me.

(1) "If under changed conditions of life a structure before useful, becomes less useful, its diminution will be favoured, *for it will profit the individual not to have its nutriment wasted in building up a useless structure*. . . . Thus, as I believe, *natural selection* will tend in the long run to reduce any part of the organization as soon as it becomes, through changed habits, superfluous, without by any means causing some other part to be largely developed in a corresponding degree" ("Origin of Species," sixth edition, p. 118).

(2) "Organs, originally formed by the aid of natural selection, when rendered useless, may well be *variable*, for their variations can no longer be checked by natural selection. . . . It is scarcely possible that disuse can go on producing any further effect after the organ has once been rendered functionless. Some additional explanation is here requisite, which I cannot give. If, for instance, it could be proved that every part of the organization tends to vary in a greater degree towards diminution than towards augmentation of size, then we should be able to understand how an organ which has become useless would be rendered, independently of the effects of disuse, rudimentary, and would at last be wholly suppressed; for the variations towards diminished size would no longer be checked by natural selection. *The principle of the economy of growth* explained in a former chapter [cited in quotation No. 1], by which the materials forming any part, if not useful to the possessor, are saved as far as possible, will perhaps come into play in rendering a useless part rudimentary" ("Origin of Species," sixth edition, pp. 401-402).

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Can it be that Prof. Lankester has not even yet perceived the significance of "the idea" of panmixia? Such certainly seems to be the case from his use of the above quotations. For the words which I have italicized render it most obvious that the only principle under consideration is the economy of growth or nutrition, *i.e.* the *reversal* of selection: there is no allusion to panmixia, or the *cessation* of selection. In the second passage it is shown that, because "no longer checked by natural selection," useless organs will become *variable;* and hence that *if there were any other cause tending to degeneration* (such as the "impoverished conditions" subsequently suggested), natural selection would *not interfere* with—*i.e.* prevent or "check"—the degenerating process thus induced. But there is no hint that the mere cessation of natural selection must be *itself,* and in *all cases,* a cause of degeneration.

Similarly, at the end of his letter, Prof. Lankester again fails to distinguish between the cessation and the reversal of selection. For, after endeavouring to represent that Mr. Darwin did not understand my "view,"[1] he says, "it is not at all surprising that Mr. Darwin did not recognize any resemblance between it and his own statement, viz. that 'the materials forming any part, if not useful to the possessor, are *saved* as far as possible,' thus 'rendering a useless part rudimentary.'" Not surprising, indeed. But it is surprising that Prof. Lankester, even at this time of day, should thus appear incapable of clearly distinguishing between natural selection as *withdrawn* and as *reversed.* For this is the whole point, and the only point so far as "the doctrine of panmixia" is concerned. It is a matter of familiar knowledge that Mr. Darwin at all times and through all his works laid considerable stress upon the "economy of growth," (or, more generally, reversed selection); but, most emphatically, this is *not,* as Prof. Lankester now says it is, "the essence of the anti-Lamarckian view of the effects of disuse." The essence of this view is, and can only be, the *cessation* of selection, as Prof. Weismann has clearly perceived.[2]

In order that there shall be no doubt upon this point, I must here explain the importance of the *cessation* of selection, as distinguished from the *reversal* of selection, in regard to "the essence of the anti-Lamarckian view"—even though in so doing "I feel it rather a severe burden when I am called upon to expound the merest commonplaces of the subject under discussion."

As stated in my previous letter, "the principal evidence on which Mr. Darwin relied to prove the inheritance of acquired characters was that which he derived from the apparently inherited effects of use and disuse—especially as regards the bones *of our domesticated animals.*" Now, the reason why our domesticated animals appeared to furnish the most unequivocal proof of the inherited effects of disuse (and so, likewise, of the inherited effects of use, as explained in my last letter) was this. In the case of all species in a state of nature, it is, as Darwin observed, impossible to eliminate the effects of natural selection (acting through the economy of growth, or otherwise) from those of disuse, supposing disuse to be a cause of degeneration in species as it is in individuals. Therefore, in order to estimate what, if any, is the proportional part that is played in degeneration by the inherited effects of disuse, it is necessary to find cases where disuse, if it ever acts at all, must be acting *alone.* Such cases Mr. Darwin took to be furnished by our domesticated animals, seeing that they are so largely protected from the struggle for existence on the one hand, while, "on the other hand, with highly-fed domesticated animals, there seems to be no economy of growth, nor any tendency to the elimination of superfluous details." Having found in such cases material for ascertaining the effects apparently caused by disuse *alone,* Darwin concluded that he was able to estimate the degree in which these effects occurred elsewhere, or generally; even though in all wild species they must usually be more or less associated with the effects of reversed selection. Therefore it was that he chose domesticated animals for all his weighings and measurings of comparatively disused parts—with the result of appearing to obtain good evidence of a high degree of reduction as due to the inherited effects of disuse alone. But it did not occur to him that the amount of reduction thus proved might be equally well explained, not indeed by the *reversal* of selection (as in wild species), but by the *cessation* of selection, or panmixia. And it is just because the cessation of selection thus applies with even more certainty to the case of domesticated animals, than does the reversal of selection to the case of wild animals, that the former principle is of such unique importance to "the essence of the anti-Lamarckian view": by its means, *and by its means alone,* can the apparent evidence of the inherited effects of disuse be overthrown.

Therefore, by seeking to assimilate the distinct principles of selection as withdrawn and selection as reversed, Prof. Lankester is performing but a sorry service to his anti-Lamarckian cause. Weismann may well cry, "Save me from my friends," when he finds them thus playing into the hands of his opponents. For on all the logical bearings of his principle of panmixia, Weismann has perfectly clear and accurate views; and although he was not accurate in representing the relations which obtain between this principle and that of reversed selection, such is but a small error compared with Lankester's identification of the two principles—with the necessary result of again bringing into court the whole body of direct evidence on which Darwin relied in his apparent proof of Lamarck's "second law."

We shall now, perhaps, be able to understand what Prof. Lankester means when he says: "The idea [of panmixia] occurred to me also shortly after the passages above quoted from Mr. Darwin were published." If this is the case, "the idea" in question must have "occurred" to Prof. Lankester before he had reached his teens, seeing that one of "the passages" in question is not confined to "the last edition of the 'Origin of Species,'" but runs through them all. Allowing this to pass, however, what I have now to remark is, that if the idea which occurred to Prof. Lankester "shortly after the publication of that work" (1872) was, as he alleges, the idea of panmixia, it becomes a most unaccountable fact that in his laborious essay on "Degeneration" (1880) there is no hint of, or even the most distant allusion to, this idea. Yet, in the presence of this idea, "Hamlet" without the Prince of Denmark would be a highly finished work compared with an essay on "Degeneration" without any mention of panmixia. Therefore, here again, I can only understand that Prof. Lankester has not even yet assimilated "the idea in question." He confounds this idea with that of the economy of growth: he fails to perceive the very "essence" of the idea, in the all-important distinction between selection as withdrawn and selection as reversed. Without question, his essay on "Degeneration" proves a familiar acquaintance with the doctrine that "the materials forming any part, if not useful to the possessor, are saved as far as possible"; but, most emphatically, this is *not* "the idea of panmixia," while it *is* the idea that is definitely "formulated" scores and scores of times through all the editions of Mr. Darwin's works—an "idea," therefore, which must necessarily have "occurred" to every reader of those works since the time when Prof. Lankester was at school.

As this letter has already run to an inordinate length, I will relegate to a footnote my discussion of the merely personal criticisms which Prof. Lankester has passed upon my former communication.[1]

GEORGE J. ROMANES.

London, March 28.

There is something comical to me in this endeavour, in view of all the conversations and correspondence which I had with Mr. Darwin upon the cessation of selection. Moreover, I do not in the least agree with Prof. Lankester where he says that my "view, as it appears in Mr. Darwin's words ('Variation,' &c., vol. ii. p. 309), is certainly *not* the same as that which Mr. Romanes has expounded in NATURE of March 13, 1890." That my "view" is not *fully* given, Mr. Darwin himself affirms; but, "as far as it can be given in a few words," it is given as correctly as I could wish.

[2] It appears to me that Prof. Lankester cannot have read Prof. Weismann's exposition of "the doctrine of panmixia." For, not only does he make this otherwise unaccountable (and, in relation to his "anti-Lamarckian view," suicidal) blunder of seeking to unite, if not virtually to identify, the principles of panmixia and economy of growth; but he alludes to Weismann as having "stated briefly" the former principle. "Stated briefly" it certainly is in "the translated essays"; but this is only because it is set out at length in one of the untranslated essays, which is entirely devoted to expounding the matter ("Ueber den Rückschritt in der Natur"). And this reminds me that in his review of Mr. Wallace's "Darwinism" there is a passage which similarly indicates that Prof. Lankester has either not read, or has strangely forgotten, another of Weismann's unpublished essays. Therefore, seeing how ready he is, on account of a precisely similar omission, to jump upon Mr. Herbert Spencer—whose recent and protracted illness is notorious—one can scarcely refrain from asking in his own words, "Will not Mr. Spencer and *others* who are interested in these matters read Weismann's essays?"

[1] Prof. Lankester says:—"As soon as the matter had taken root in his mind, Mr. Romanes published in NATURE, March 12, April 7, and July 2, 1874, an exposition of the importance of the principle of cessation of selection as a commentary upon a letter by Mr. Darwin himself (NATURE, vol. viii. pp. 432, 505), in which Mr. Darwin had suggested that, with organisms subjected to unfavourable conditions, all the parts would tend towards reduction. Mr. Darwin, with his usual kindly manner towards the suggestions of a young writer, gives, at p. 309 of vol. ii. of 'Animals and Plants under Domestication,' Mr. Romanes's view, 'as far as it can be given in a few words.'" Now, as it is only a few days ago that I myself directed Prof.

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